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## Comment on acp-2021-579

Anonymous Referee #3

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Referee comment on "Impact of the COVID-19 pandemic on the observed vertical distributions of PM<sub>2.5</sub>, NO<sub>x</sub>, and O<sub>3</sub> from a tower in the Pearl River Delta" by Lei Li et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-579-RC3>, 2021

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This manuscript reports the vertical distributions of PM<sub>2.5</sub>, NO<sub>x</sub> and O<sub>3</sub> at a tower in Pearl River Delta, China, before and during the COVID-19 lockdown, and analyzed the variations of these pollutants from different aspects and tried to give the responsible reasons. The authors finally concluded that the reductions of anthropogenic emissions by the lockdown were effective in mitigating both PM<sub>2.5</sub> and O<sub>3</sub>. Although the datasets provided in this manuscript are unique and interesting, the organization and analysis is not sound and scientific. Many explanations for the observed data trends were too arbitrary, with little or even no evidence. Importantly, the role of meteorology on the observed trends was underestimated or even neglected. In addition, despite no PM<sub>2.5</sub> composition data in this manuscript, a large amount of papers on PM<sub>2.5</sub> in the PRD were not cited to support the data analysis. In my point of view, this is a very primary manuscript and no solid science has been drawn. It still has a long distance to publication at a high-level journal like ACP or similar journals, and thus I recommend rejecting it.

### Major comments:

1. There seem to be more pollutants which are routinely monitored in China's air quality network, such as SO<sub>2</sub>, PM<sub>10</sub> and CO. Why these data are not included? They can provide more useful information on primary emissions. In addition, I don't understand why the authors included the 2017 data. As the authors said, both the emissions and weather in 2017 were different from those in 2020.
2. Since meteorology largely influences or even dominates the variations of air pollutants, analysis of major meteorological parameters must always accompany the explorations of the variations of air pollutants. The comparison of averages of meteorological elements in Table 1 is far less than enough. Line 251 "it can be concluded that the meteorological conditions of the Shenzhen region were largely identical before and during the lockdown", this is too arbitrary. Since the vertical profiles/diurnal variations of the air pollutants in this study are not large (in Fig. 7), the simultaneous variations of temperature, RH and wind should be seriously analyzed. Unfortunately, meteorological analysis seemed to be totally forgotten starting from Section 3.2.
3. Section 3.3. Why the authors did not include the ground data in this section? The

current vertical trends were represented by only three altitudes. If you add ground data in Fig. 7, will the vertical trends be the same?

4. There are too many subjective inferences without support or citation in the data analysis, such as:

Line 267: "During night and early morning, the height of the mixing layer top is between 120m and 220m, so the curves of the upper and lower layers are quite different." Any evidence for the height of the mixing layer top?

Line 270: "This may have been caused by the presence of dense forests near the ground observation point (Shiyan Base), which may have obstructed the dispersal of particulate matter and thus reduced the apparent PM<sub>2.5</sub> concentration." I think wind vertical profile may also influence it.

Line 275: "High-height PM<sub>2.5</sub> is formed predominantly by chemical reactions, whereas low-height PM<sub>2.5</sub> may be derived from multiple sources (predominantly surface-level primary emissions)." This sentence is not sound. In the literature, secondary aerosols account for the major part of PM<sub>2.5</sub> in PRD even at the ground level. The influence of regional transport of secondary aerosols at different altitudes was not discussed here.

Line 288: "It is likely that the morning peak of the pre-lockdown curve was caused by direct emissions from nearby human activities. These emissions were therefore greatly reduced by the lockdown-mediated decrease in human activity and were more easily blocked by the dense forest around the ground observation point." Such analysis is too subjective.

Line 312: "Since NO<sub>x</sub> is a primary pollutant, its significantly lower concentrations at the low altitudes implies that near-ground chemical reactions consume it more rapidly than the high-altitude chemical reactions do." What reactions? Why do they consume NO<sub>x</sub> more rapidly?

Line 333: "A possible reason leading to this phenomenon is that in the area where the SZMGT is located, the key height of night chemical reactions may be around 110-120 m." Such analysis is irresponsible.

Lines 357-370. The discussion here is generally weak. How about PM<sub>2.5</sub> at the ground? What is the role of wind profile and/or regional transport? The authors may firstly check whether all vertical differences are statistically significant, especially when considering the measurement accuracy. "At the middle level and above, PM<sub>2.5</sub> is formed mainly by photochemical 360 reactions (Li et al. 2020)", such citation is invalid. The vertical profile in Taiwan could be totally different and cannot support the discussion.

Line 413: "It may be inferred that prior to the lockdown, PM<sub>2.5</sub> and O<sub>3</sub> did not have related sources. However, during the lockdown, both were likely to have a similar source." Such discussion is too casual.

Line 423: "At lower heights, a considerable part of PM<sub>2.5</sub> is primary source and had nothing to do with photochemical reactions", such analysis is irresponsible.

Line 431: "the primary aerosol like black carbon is not reduced," where are the black carbon data?

Minor comments:

Lines 272-273: what are the reasons for the peak occurring at 17:00–19:00?

Line 298: a typo for mush.

Line 326: the high value can last to about 18:00, not only for 8:00 to 10:00.

Lines 347-349: how to calculate nitrate radical production rate?

Lines 406-411: such basic description is not necessary.

Figures 9 and 10: the correlations were also affected by the data range and amount.

Lines 462-464: this sentence contradicts itself and should be rewritten.